The Unexpected Effects of No Pass, No Drive Policies on High School Education

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15 October 2018

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October 15, 2018

Abstract

Since 1988, 27 states have introduced No Pass, No Drive laws, which tie a teenager's ability to receive and maintain a driver's license to various school-related outcomes – most commonly, enrollment and attendance. *Truancy-Based* No Pass, No Drive policies target only attendance – teens that fail to meet a minimum attendance requirement lose their driver's license. However, these policies allow students to drop out of school without facing this penalty. These policies increase the annual dropout rate by between 32 and 45 percent (1.4 to 2 percentage points). *Enrollment-Based* No Pass, No Drive policies, the largest group of policies, which target both enrollment and attendance, have negligible effects on dropout rates, but decrease the Averaged Freshman Graduation Rate (AFGR) by more than one percentage point. However, this lower graduation rate stems from students delaying their dropout decision by up to two years. As a result, these students are retained in the ninth and tenth grades, increasing ninth grade enrollment by 2.8 percent relative to eighth grade enrollment the year prior; this causes an artificial reduction in the graduation rate, rather than a reduction in the true likelihood that a student will graduate.

(JEL: H75, I28)

Keywords: AFGR, Dropout Rate, Driver's License, Education, No Pass No Drive

*I thank Timothy Bond, Jillian Carr, and Kevin Mumford for helpful feedback and discussions. I also thank conference participants at the Midwest Economics Association 2016, Society of Labor Economists meetings 2016, Association of Public Policy and Management International Conference 2016, the European Association of Labor Economists 2016 for many helpful comments and discussions.

1 Introduction

Over the past 30 years, attempting to lower dropout rates and raise graduation rates has been a central focal point of education policy on both the state and national level. At first glance, this increased attention has improved these two educational outcomes – The National Center for Education Statistics' (NCES) *Trends in High School Dropout and Completion Rates in the United States: 1972–2012* reports that the "status dropout rate," or the proportion of 16-24 year-olds who are not enrolled in high school and do not have a high school diploma or equivalency certificate, fell from 14.6 percent in 1972 to 6.6 percent in 2012. Likewise, the proportion of 18-24 year olds who have completed high school or an equivalent program has risen from 82.8 percent in 1972 to 91.3 percent in 2012. However, this rise in high school completion is predominantly due to an increase in GED recipiency. Studies such as Cameron and Heckman (1993) and Heckman et al. (2011) show both that the value of equivalency certificates such as the GED is far less than the value of a high school diploma, and that the rate of GED recipiency has risen dramatically over the past 30 years. In addition, Haney et al. (2004) show that the on-time graduation rate fell from 79% in the early 1980s to 74.4% in 2000-01. As a result, public policymakers have shifted their focus from ensuring teens complete high school or an equivalency program to ensuring teens graduate from high school on time. This study focuses on No Pass, No Drive (NPND) laws – a widespread, low-cost incentive program tying teens' ability to receive and maintain a driver's license to their enrollment, attendance, behavior, and/or performance in school – and examines how these laws affect dropout rates, on-time graduation rates, and grade-by-grade enrollment across the country.

In 1988, West Virginia enacted the first No Pass, No Drive law in response to growing problems with dropout rates and graduation rates, especially in rural areas. West Virginia, as do most states with NPND policies, mandates that teenagers must be enrolled in and attending school regularly in order to receive and maintain a driver's license. When students fall below a minimum attendance threshold, or withdraw from school entirely, the school contacts the state's licensing office and instructs them to suspend the teen's license. Early anecdotal evidence suggested that NPND policies were successful in their goal of keeping teens in school. In a 1989 New York Times article,[1] West Virginia school officials credited the enactment of the state's NPND law with a 1-2 percent reduction in the annual dropout rate (from approximately 5 percent to 3.4 percent). Following the early results in West Virginia, 26 more states enacted their own versions of NPND policies in an effort to recreate the early success of West Virginia's policy. I look at the enactment of these 27 policies, exploiting variation in the timing of their enactment between states and examining differences between groups of these policies, to identify the causal effects of these policies on various educational outcomes.

A small group of No Pass, No Drive laws are aimed at preventing truancy – a student who is enrolled in school and habitually absent will lose his or her driver's license. Importantly, however, these teens can drop out of school without penalty, and a teen who previously lost his or her license due to this law can even have their license reinstated (after a waiting period in some states). I show that this group of policies increases the event dropout rate, or the proportion of students who drop out of school in a single academic year, by between 1.4 and 2 percentage points. These policies also lower the Averaged Freshman Graduation Rate (AFGR) by 5 percentage points. In all of the states in this policy group, students can first receive their driver's license at age 16 and can first drop out of school at age 16, so these policies should have a greater impact on 10th graders than on other grades in high school, as students typically turn 16 in their 10th grade year. I show that, from October of 10th grade to October of 11th grade the following year, enrollment falls by 2.7 percent more than expected in these states, indicating that students are responding to the threat of having their license revoked by dropping out of school immediately upon turning 16.

The largest group of policies, in 20 states, targets both truancy and enrollment – these states close the "dropout loophole" mentioned above and require students to be enrolled in school and attending regularly in order to receive and maintain a driver's license. As expected, these policies do not increase the dropout rate, having no effect on the likelihood a student will drop out of school.

[1] "West Virginia Reduces Dropouts by Denying them Driver's License." *The New York Times,* May 21, 1989.

However, these policies cause a 1.3% *reduction* in the AFGR, an unintuitive result. I investigate two potential mechanisms for this graduation rate decrease. First, this could be caused by a reduction in the true four-year graduation rate; that is, these policies could reduce the likelihood a teen will graduate with a regular high school diploma within four years. Alternatively, since the AFGR accounts for 9th and 10th grade enrollment in its denominator, it could fall as a result of students *delaying* their dropout decisions; a student who would drop out at the age of 16 in the absence of NPND laws may instead drop out at 18 in order to keep his or her drivers' license. If these delayed dropouts are retained in the 9th and/or 10th grades, then the dropout rate would be unaffected overall, but the denominator of the AFGR would be larger, causing a decline in the AFGR. I find that NPND policies cause 9th grade enrollment to be 2.8 percent larger than expected, given 8th grade enrollment the year prior, suggesting that NPND policies delay student dropout decisions. I additionally test alternative graduation rate estimates that do not depend on 9th and 10th grade enrollment, and find no evidence to suggest that the true four-year graduation rate is affected by NPND policies. Thus, while the "Truancy-Based" policies increase the true dropout rate and decrease the true likelihood a student will graduate from high school, the "Enrollment-Based" policies that target truancy and enrollment cause an *artificial reduction* in the graduation rate, distorting the perceived quality of education in these states without actually directly affecting the true quality of education.

My results also inform the literature on behavioral incentive responses, particularly in relation to education incentive schemes. A number of studies have examined positive educational incentive schemes, such as Mexico's PROGRESA (Behrman et al. 2005) and Colombia's PACES (Angrist et al. 2002), finding that providing monetary incentives for attendance and enrollment successfully increase both attendance and enrollment. However, these policies are quite expensive to maintain, so many policymakers have turned to *negative* incentive schemes in attempts to boost enrollment and attendance. These policies penalize truancy and/or dropping out of school, and typically have much lower monetary costs to implement and maintain them. One negative incentive policy, Wisconsin's Learnfare policy, penalizes students who drop out of school or fail to meet an attendance target by sanctioning the family's welfare grant. Dee (2011) finds that Learnfare had large effects on enrollment and daily attendance when properly implemented, and ambiguous effects when poorly implemented. No Pass, No Drive laws are an example of a widespread, low-cost negative incentive scheme, where the incentives are poorly aligned. Both Behavior-Based and Truancy-Based NPND policies have easily exploitable loopholes where teens can avoid the penalty without modifying their behavior in the manner lawmakers intended. This creates an interesting comparison – positive incentive schemes with misaligned incentives would typically result in wasted money; teens could gain the reward without modifying their behavior. However, negative incentives schemes with misaligned incentives may motivate students to modify their behavior in undesirable ways. No Pass, No Drive policies provide a clear example of this rarely-noticed risk of negative incentive policies – in trying to prevent students from dropping out of school or skipping school, the two main types of NPND policies cause large increases in the 9th grade retention rate and the annual dropout rate, respectively.

Only three other papers have empirically examined No Pass, No Drive policies previously.[2] Krimmel (2000) performs a time-series analysis of the initial rollout of these policies in Kentucky, finding that counties enacting NPND policies saw an 11 percent reduction in their dropout rate, while counties not enacting these policies only saw an 8 percent reduction. More closely related to my study is Barua and Vidal-Fernandez (2014), who focus on the effects of NPND policies nationwide on educational attainment and student time use. They show that students decrease leisure and work hours and increase time allocated to schoolwork. They connect this to an increase in the fraction of men and black individuals with high school diplomas, as well as increases in completion rates of grades 10 and 11. A second working paper by Barua and Vidal-Fernandez (2016) looks at the effects on juvenile crime, finding that males aged 16 to 18 show a reduction in violent, drug-related, and property crime as a result of NPND policies. My analysis, in contrast to the previous literature, is the first national study on how heterogeneous NPND policies affect their targeted educational outcomes – dropout rates and on-time graduation rates, as well as grade-by-grade re-

[2]These papers are discussed in greater detail in Section 2.2

tention rates – using data measured contemporaneously with the policies' enactments.

My analysis of No Pass, No Drive policies makes three main contributions to the literature on NPND laws, and, more generally, incentive programs and their effects on teen behavior. First, I perform the first analysis of the effects of NPND policies on their targeted outcomes – dropout rates and graduation rates. Second, I am the first to examine how students respond to NPND policies differently at each grade level.[3] This study is the first to explore the heterogeneous education requirements of NPND laws, and is the first national study to analyze NPND effects contemporaneously with their implementation. Third, in my grade-by-grade examination, I demonstrate an issue with using the AFGR, and by extension, any graduation rate estimate that does not track students year-to-year, as a measure of policy outcomes – that graduation rates can be significantly biased in the presence of policies and programs that distort enrollment. Specifically, I show that rather than reducing the number or true rate of graduates, which would contradict the existing literature, Enrollment-based NPND policies increase enrollment in 9th and 10th grades relative to their initial cohort size in 8th grade. Truancy-based NPND policies, however, cause students to drop out of school and reduce the number of graduates per year. This provides an interesting scenario – both Enrollment- and Truancy-based NPND policies lower graduation rate estimates, but the decrease in the Enrollment-based states does not stem from a reduction in the *true* graduation rate, while the decrease in Truancy-based states does. This peculiarity with Enrollment-based NPND policies is also not unique to the AFGR; any estimate that includes 9th and/or 10th grade enrollment in the denominator will be negatively affected by this type of NPND policy. The AFGR was the benchmark graduation rate statistic used by the U.S. Department of Education from 2004-2012 under the No Child Left Behind Act (NCLB) to determine Adequate Yearly Progress (AYP). NPND policies, by lowering the AFGR, decreased the likelihood of schools, districts, and states of meeting their respective AYP standards, and likely resulted in a number of schools being designated for "improvement" that, in the absence of NPND policies, would have otherwise met their graduation rate goals.

[3]Barua and Vidal-Fernandez (2014), due to their data structure, cannot examine how promotion and retention behavior changes within each grade level. They can only examine students' final educational attainment.

The remainder of this paper is organized as follows: Section 2 explains the history of No Pass, No Drive policies, including a list of each policy across the country and their attributes, as well as a review of the relevant literature. Section 3 describes the data sources and estimation strategy employed. Section 4 presents the primary results, explains the forces driving my results, and presents a set of robustness checks to demonstrate validity of my findings, as well as to show a lack of alternative explanations for my results. Section 5 discusses the impact of No Pass, No Drive policies under the No Child Left Behind Act and provides concluding remarks.

2 Background

2.1 Policy Details

Starting with West Virginia in 1988, 27 states have passed laws linking student behavior, attendance, and performance to their ability to receive and maintain a drivers' license. Differing from the previous work on these policies, I categorize these policies into groups to more closely examine the effectiveness of the various incentives. For example, the first of these policies, in West Virginia, only allows a license to be held by a student who *"maintains current school enrollment and is making satisfactory academic progress."*[4] The policy in Nevada states that *"If a child is adjudicated to be in need of supervision because the child is a habitual truant, the juvenile court shall . . . order the suspension of the driver's license of the child"*[5] The policy in West Virginia requires school enrollment and "satisfactory academic progress," while the Nevada policy only requires the student not be a truant. This allows teens to change their behavior in Nevada to avoid punishment under this policy; truant teens can drop out of school and no longer be classified as truant, thus allowing them to maintain their license. Additionally, a third type of policy in Kansas states that *"Whenever a pupil who has attained the age of 13 years has been found in possession of a weapon or illegal drug at school, . . . the division of vehicles immediately shall suspend the pupil's driver's license or privilege to operate a motor vehicle."*[6] Clearly, these are three different

[4]West Virginia Annotated Code, §17B-2-3a(2)(E)
[5]Nevada Revised Statutes, §62E.430
[6]Kansas Statute Annotated, §72-89c02(a)-(c)

policies, attempting to curb different problems, and so examining them as if they were a single policy aimed at reducing dropout rates and raising graduation rates would inaccurately identify the full effect of those policies that do target these rates. To solve this, I classify the policies into three groups – *Enrollment-Based* policies, as in West Virginia, which directly target high school dropouts and truants, *Truancy-Based* policies, as in Nevada, which target truants but not dropouts, and *Behavior-Based* policies, as in Kansas, which target school discipline. These groupings, as well as some unique features of these policies, are shown in Table 1.

Except for the case of Kentucky (see footnote 25), no NPND policy has ever been repealed; all 27 of these policies are currently ongoing. Implementing these policies requires coordination between local education agencies and the drivers' license issuing offices. For example, some states, such as Kentucky, have an electronic communication system in place between schools and the Kentucky Division of Motor Vehicle Licensing, so that a student failing to meet the requirements to keep his or her license at school will be automatically "flagged" in the system (Krimmel 2000). In contrast, states like Texas have no such electronic communication system in place. Instead, students in Texas must take a form confirming their enrollment and attendance for the past 180 days in to the Texas Department of Public Safety in order to receive a learner's permit at age 15, to receive a graduated drivers' license at age 16, and to renew the license at age 17 (TX Transportation Code 7B-521-003). Thus, heterogeneity across states in reporting student activity and in enforcing these policies is present. However, due to the subjectiveness of attempting to categorize these systems, as well as uncertainty on how such electronic systems have been implemented over time, I am unable to examine the effects of differing reporting systems on student outcomes.

I am also unable to directly link enforcement of these policies to student outcomes, as data on revocation and suspension of licenses under NPND policies are generally unavailable. A potential concern would be that the policy (i) does not affect enough teens, or (ii) is not strictly enforced, so that the impact on education statistics for the entire population of high school students would be imperceptible. However, a 2006 nationally representative survey of teen drivers estimates that nearly 75% of 9th-11th graders drive regularly, either on their own or with a driving instructor or parent (*"Driving Through the Eyes of Teens..."* 2009). By 11th grade, only 10% of teens do not drive at all, suggesting No Pass, No Drive policies would affect almost all high school students. The relevant results of this survey are reproduced in Table A3 in the appendix. Additionally, although enforcement numbers are not available in most states for most years, a 2011 policy brief from the Southern Regional Education Board reports these numbers for four states (Lenard 2011). The full set of enforcement numbers available is reproduced in Table A4 in the appendix, as well as counts of driver's licenses held by teens in those states from the US Federal Highway Administration in 2009. Tennessee has the lowest annual suspension rate, at 2.8% of licenses suspended per year. The other three reporting states suspend over 10% of all teen licenses per year, suggesting these policies are generally enforced, but with some heterogeneity.[7] Although I cannot test the degree of enforcement of these policies directly, it appears that No Pass, No Drive policies are enforced and, more importantly, that teens in the United States drive regularly.

2.2 Relevant Literature

Only three studies, to my knowledge, have empirically examined the effects of No Pass, No Drive policies. Krimmel (2000) was the first to perform an empirical study of the effects of No Pass, No Drive policies on educational outcomes. His time-series analysis of a quasi-natural experiment in Kentucky showed that counties that enacted NPND laws saw an 11 percent reduction in the dropout rate, while the comparison group of counties that did not enact NPND laws only saw an 8 percent reduction in the dropout rate. My analysis differs from Krimmel's in a few key dimensions. First, my analysis is on a national level, exploiting timing differences in the enactment of NPND policies to identify their effects, while Krimmel's only identifies the effect of Kentucky's NPND laws. Thus, my results should provide greater external validity in informing policy decisions. Second, Krimmel only investigates the effects of NPND laws on dropout rates, while I am able to identify the effects of NPND laws on four-year graduation rates, individual grade enrollments, and dropout rates. Finally,

[7]Florida suspended 3.4% of all licenses in the 3rd quarter of 2009, for an estimated annual suspension rate of 13.7%.

and most importantly, Krimmel's policy analysis is conflated with the simultaneous enactment of dropout prevention counseling programs in the treatment counties, meaning that Krimmel's results should be interpreted as the effect of NPND laws in conjunction with dropout prevention counseling, while my analysis, by examining these policies nationwide and exploiting heterogeneous timing of NPND enactment, is able to identify the impact of NPND policies separately and more accurately.

The only other works to empirically study No Pass, No Drive policies are two studies by Barua and Vidal-Fernandez (2014, 2016). Their 2014 study examines NPND policies on a national level, primarily focusing on the effects of NPND policies on student time use and educational attainment. Using the 2009-11 rounds of the American Community Survey (ACS), they assign treatment to individuals based on their age and state of birth. Individuals born in a state with a NPND law who were under the age of 13 when the policy was enacted are considered treated, and individuals aged 19 or older are untreated.[8] Individuals aged 14 to 18 in the year their state's NPND law was enacted are omitted from the estimation. The authors perform difference-in-differences estimation identified using between-state and between-birth cohort variation. They show that NPND policies increase the proportion of young adults who have a high school diploma by approximately 1 percentage point, and demonstrate stronger effects on males (1-2%), blacks (1-4%), and particularly black males (3-7%). They also demonstrate that educational attainment increases by 0.02 to 0.05 years on average, and that students spend approximately 0.2 more hours per week on schoolwork as a result of NPND laws.

My work has two clear advantages over that of Barua and Vidal-Fernandez (2014). First, I can identify the educational effects of these policies contemporaneously with their implementation. My data are taken from annual reports from the National Center for Education Statistics (NCES), meaning that I can identify which state-year pairs are treated in a much cleaner manner than is possible in the ACS.[9] Barua and Vidal-Fernandez omit individuals aged 14-18 when the NPND

[8] The earliest age at which a person could be affected by a NPND policy is 13, as Alabama, California, and Kansas provided learner's permits at age 13 when these NPND laws were enacted.

[9] A potential threat to this strategy is how to assign treatment when policies are enacted during an academic year. I use October 1st as the cutoff date for treatment, as enrollment and dropout counts are measured nationwide in

policy was enacted, meaning that any effects in the first three to five years of an NPND policy are unobservable in their study. Second, I separate NPND policies into three types based on the group of students they target. As previously mentioned in Section 2.1, some NPND policies target both dropouts and truants, some target only truants, and some target neither; my study is the first to identify these policies as potentially having different effects. Furthermore, my study is primarily a policy analysis – I investigate whether NPND policies are achieving their goals of increasing on-time graduation rates and lowering dropout rates, while Barua and Vidal-Fernandez look only at the final educational attainment of teens, then examine the behavioral responses of teens to these policies. However, though Barua and Vidal-Fernandez show a number of unambiguous positive effects of NPND policies, I demonstrate that the largest group of laws cause an increase in grade retention in 9th and 10th grades that exceeds any increase in the number of graduates. My estimates suggest that two to three times more students are retained in the 9th and 10th grades than Barua and Vidal-Fernandez find graduate due to NPND policies.[10] I also show that a smaller group of laws is extremely problematic, causing a substantial number of students to drop out of school entirely. Despite these differences, my results do not directly contradict any of Barua and Vidal-Fernandez's findings; it is likely that NPND policies moderately increase high school completion and educational attainment, but cause large increases in 9th grade retention.

Barua and Vidal-Fernandez (2016) look at the effects of No Pass, No Drive policies on juvenile crime. Using the FBI's Uniform Crime Reports, they examine how the passage of an NPND policy affected violent crimes, property crimes, drug crimes, and DUI offenses for teenage males and females aged 16-18. This study examines NPND policies contemporaneously, unlike their 2014 study, and compares the effects of NPND policies to the well-known effects of compulsory schooling laws on crime (Lochner and Moretti 2004, Anderson 2014). Using a difference-in-differences framework with state-specific linear trends, they show that both males and females have a 0.06 percentage point decrease in crime, primarily driven by drops in violent crime (0.11 percentage points) and property

early October, and reported to the NCES in mid- to late October.

[10] Barua and Vidal-Fernandez's results suggest that for every 1,000 students, an additional 12 will graduate due to NPND policies. I show in Figure 1 that for every 1,000 students under an Enrollment-based policy, an additional 34 will be retained in 9th and 10th grades due to NPND policies.

crime (0.1-0.13 percentage points). They then perform a triple difference, using variation in 20-24 year olds' crime rate as a comparison group to remove potential state-year level crime shocks, finding similar, but larger results. My results complement these results, suggesting retention and incapacitation as potential mechanisms for the reduction in crime rates among teens affected by NPND laws.

3 Data and Methods

3.1 Data Sources

I collected information on the details of these policies from each state's code of laws and legislative histories. I then merged this policy information with data on student outcomes and school spending taken from the National Center for Education Statistics. The NCES's Common Core of Data (CCD) has public-use state-level data on dropout rates, graduation rates, diploma counts, and enrollments.[11] Data on grade enrollments from grade 8 to grade 12, as well as total diploma recipients, were taken from the State Nonfiscal Public Elementary/Secondary Education Survey datasets available on the NCES website. I assembled a panel dataset from the state-level enrollment files from 1986-87 to 2012-13. The data are missing the number of diplomas in 1986-87, as well as from 2010-11 to 2012-13. For school years from 2005-06 to 2009-10, data on the number of diploma recipients in each state were taken from the CCD's State Dropout and Completion Data Files.

Pre-constructed graduation rates are unavailable in a consistent and accurate form across all years of my dataset. With the passage of the No Child Left Behind Act (NCLB) in 2001, the US Department of Education (DoE) mandated that states begin implementing more accurate measures of four-year graduation rates. As a result, attempting to link the NCES's graduation rates, which are self-reported by each state, and do not follow a consistent formula, before and after 2001 would

[11] Although district-level data are available, they only begin in 1997 and are highly incomplete, and as such are not used in my analysis.

create severe inconsistencies in my analysis. To circumvent this issue, I use the Averaged Freshman Graduation Rate (AFGR), proposed by Greene and Winters (2002), which is an easily calculable graduation rate statistic that only requires state-level grade enrollment data. The AFGR was the standard graduation rate used by the US DoE from 2005-2012 in determining school- and district-level adequate yearly progress under NCLB, and is generally considered to be a good estimate of on-time graduation rates, given limited available data.[12] The AFGR is calculated for each state (s), in every year (t) using the formula below:

$$AFGR_{s,t} = \frac{Diplomas_{s,t} * 3}{Grade\ 8_{s,t-4} + Grade\ 9_{s,t-3} + Grade\ 10_{s,t-2}} * 100$$

Simply put, the AFGR uses the average enrollment for a single cohort from 8th to 10th grade as the "enrollment base" from which the four-year graduation rate is calculated. As a result of this formula and the available data, the AFGR is calculated from 1990 to 2009. I use the AFGR, as opposed to educational attainment in the full population as in Barua and Vidal-Fernandez, to provide a policy analysis more aligned with the interests of policymakers and educational agencies. The US DoE's Race to the Top program, implemented in 2011, challenges states to achieve four-year graduation rates of 90% by the year 2020, so a four-year graduation rate is a more appropriate statistic to study than overall educational attainment in a state-level policy analysis such as this.[13]

Dropout rates were taken from the NCES's annual *Dropout Rates in the United States* reports. The NCES uses an *event dropout rate* definition, which is calculated as the total number of students that have dropped out of grades 9-12 since October of the previous academic year, divided by total enrollment in grades 9-12 in that year. This means event dropout rates have a built-in lag structure, where the event dropout rate reported in October of academic year t is primarily made up of students who dropped out of school in academic year $t - 1$. This is in contrast to other agencies that report *status dropout rates*, which are the fraction of people of schooling age (typically

[12] See Heckman and LaFontaine (2012) for an overview of various graduation rate statistics used in the United States, and a comparison of the AFGR to graduation rate estimates from other sources.

[13] On-time graduation rate estimates also have the advantage of being measured contemporaneously with NPND policy implementation, while educational attainment must be measured after individuals complete their education.

ages 15-21) that are not currently enrolled in school and have not completed school. Since these No Pass, No Drive policies often have grandfathering clauses, where students that have already dropped out of school before the policy takes effect do not lose their licenses, the event dropout rate is a more appropriate statistic for examining the early years of the policy. Additionally, status dropout rates typically consider individuals up to age 21, while NPND policies only affect those under the age of 18. Event dropout rates are used from 1990 to 2011, as these are the only years for which data are available. Additionally, approximately 20% of the observations in this time period are missing. The states with NPND policies are missing fewer observations, however – approximately 15%. I also took data on total expenditures on public schooling per pupil from the NCES's Elementary/Secondary Information System (ELSi), and state unemployment rates were taken from the Bureau of Labor Statistics from 1987-2013. All 50 states are used in this study; data are missing for some state-year pairs' dropout rates and diploma counts, but never for grade-by-grade enrollment. Summary statistics for the dependent variables and controls are listed in Table 2 below.

3.2 Estimation Methods

I aim to identify the impact of No Pass, No Drive laws on state average dropout rates and graduation rates. This is done by exploiting variation in the time of passage of these laws among the twenty states with Enrollment-Based policies, the four states with Truancy-Based policies, and the three states with Behavior-Based policies. The naïve regression model of interest is below:

$$Y_{st} = \beta_0 + \beta_1 EnrollBased_{st} + \beta_2 TruancyBased_{st} + \beta_3 BehaviorBased_{st} + \varepsilon_{st}$$

Above, the variables Enroll-Based, Truancy-Based, and Behavior-Based are binary – $EnrollBased_{st}$ is set equal to 1 if state s has an Enrollment-Based policy in place at time t and equal to 0 otherwise, and likewise for Truancy- and Behavior-Based. The outcome variable, Y_{st}, is either the dropout rate or AFGR, depending on the regression.[14]

[14]Given the timing involved in calculation of the AFGR, the first 2 years of NPND policies will only affect the numerator (number of diplomas), not the denominator (enrollment base). However, *ex ante* there is no justifiable

In order to account for differences in educational quality and economic climate between the states with and without these policies, I include a pair of control variables mentioned previously – per-student public schooling expenditures and state unemployment rates. I also include state and year fixed effects to control for baseline differences between states and the overall downward trends observed in both dropout rates and graduation rates over the past 25 years. Finally, I include state-specific linear time trends to correct for additional state-level trends in dropout rates and graduation rates differing from the overall trend. A cursory glance at the data suggests that states that initially have low dropout rates and/or graduation rates saw those numbers fall to a lesser extent than states with high rates.[15] I perform difference-in-differences estimation with state-specific linear trends using the regression model below:

$$
\begin{aligned}
Y_{st} = &\beta_0 + \beta_1 EnrollBased_{st} + \beta_2 TruancyBased_{st} + \beta_3 BehaviorBased_{st} \\
&+ \beta_4 log(Spending)_{st} + \beta_5 Unemp_{st} + \delta_t + \gamma_s + \gamma_s * t + \varepsilon_{st}
\end{aligned}
$$

Because I include state and year fixed effects and state-specific linear trends in this regression model, this estimating equation is well-identified given the "parallel paths" assumption – that the states without the policy represent a valid counterfactual for the growth path of the outcome variable in states enacting the policy. The potential threat to identification then, would be within-state deviations from their linear trends that are correlated with, but not caused by, the implementation of No Pass, No Drive policies. The coefficients β_1, β_2, and β_3 are the parameters of interest in this regression, and, assuming the identifying assumption above holds, should be interpreted as the causal effect of these No Pass, No Drive policies on dropout rates and graduation rates.

reason to reassign the start dates of treatment, as NPND policies have the potential to affect both the number of diplomas and the enrollment base.

[15] Section 4.2 further discusses why my preferred specification includes state-specific linear time trends.

4 Results

Table 3 shows the primary regression results, using the model specification described in Section 3.2 and the dropout rate as the dependent variable. In Table 3 and the remainder of the paper, all reported standard errors are the typical cluster-robust standard errors. However, the Enrollment-based policies use the typical t-distribution in hypothesis testing, while the Truancy- and Behavior-based policies use a t-distribution generated from the bootstrap procedure described in Cameron et al. (2008).[16]

Column (1) shows the naïve regression results on dropout rates, demonstrating that the states with the No Pass, No Drive policies have below-average dropout rates. This suggests that a difference-in-differences approach, as in columns (2)-(5), is necessary to establish causality. Columns (4) and (5) include the full set of controls, and are the main results. It is important to note here that, based on the event dropout rate definition, the dropout rate would only be temporarily influenced by students *delaying* their dropout decision. So, for example, if a student subject to one of these policies at age 16 decides to drop out of school at age 18, when they no longer are affected by the policy, the policy coefficient would remain unchanged. Looking at columns (4) and (5), Enrollment-Based policies have no significant effect on dropout rates, indicating that any students who do change their dropout behavior based on this policy are delaying their dropout decision. Truancy-Based policies, alternatively, increase the dropout rate by 1.229 percentage points per year – the mean dropout rate is 4.42%, so approximately 32% more students drop out of school each year than would have otherwise. Recall the incentives at work in these policies – a habitual truant will lose his or her driving privileges, but a dropout will not in these states. Additionally,

[16]Section A in the appendix describes inference for Truancy- and Behavior-based policies. In Table A1 in the appendix, placebo testing shows that analysis on the truancy-based policies suffers from severe type I error; to correct for this, I use the wild cluster bootstrap-t as in Cameron et al. (2008). This method corrects for type I error in difference-in-differences estimation when implementing clustered standard errors with rarely-treated clusters, and allows for proper statistical inference. The cluster-robust standard error estimates are presented, as opposed to a bootstrapped standard error estimate, because the cluster-robust estimate is used to calculate the Wald statistic used for inference. The bootstrap procedure tests the typical Wald statistic generated from OLS estimation against a bootstrap t-distribution of Wald statistics, so the appropriate standard error to report is the typical cluster-robust standard error estimate. The coefficients and standard error estimates presented in all tables are unaffected by the bootstrap procedure, and only the p-values and significance indicators are affected for the Truancy- and Behavior-Based policies.

as mentioned in the description of my data previously, the event dropout rate has a built-in lag, meaning my estimates of the policy effect on dropout rates likely face attenuation bias – the first year of treatment in my analysis often includes the majority of an academic year in which the policy was not in effect. To test this, and to confirm that the first-year effect is not having an unexpected impact on my results, in Table A5 in the appendix I use a "Donut Treatment" specification where I omit the first year of treatment from my analysis.[17] This specification suggests the Truancy-based policies increase the event dropout rate by 1.98 percentage points. I interpret these two estimates of the treatment effects as bounds on the true effect – that Truancy-based policies increase the dropout rate by between 1.4 and 1.98 percentage points, or 32 and 45 percent. Additionally, Figure A1 in the appendix shows the raw dropout rates and the rates controlling for all covariates in the main regression for the three states used for identification. These graphs indicate the effect is primarily occurring in Delaware and New Mexico, with less dramatic effects in Nevada.[18]

Table 4 shows the primary regression results on Averaged Freshman Graduation Rates. Again, column (1) shows the naïve regression results, demonstrating that the states with No Pass, No Drive policies have below average AFGRs. Interestingly, however, after including the full set of controls, column (4) shows the treatment effect of the policy is still negative, implying that these policies are reducing graduation rates in some meaningful way. Additionally, adding the spending and unemployment controls in moving from column (3) to column (4) increased the magnitude of the coefficient estimate and decreased the standard error, suggesting that omitted variable bias is unlikely to be the force driving this surprising result. Moving from column (4) to (5) removes the significance from the enrollment-based coefficient, with only a small change on the point estimate. This is likely driven by Louisiana and Oregon, as they switch from Behavior-based policies to Enrollment-based policies, rather than from no policy to Enrollment-based policies. Table A6 excludes Louisiana and Oregon from this analysis, and shows an almost identical Enrollment-based coefficient to that in column (4), suggesting the effect here is only marginally significant, but still

[17] This is preferred to simply reassigning treatment one year later, as the first year of treatment is partially treated. Reassigning treatment would likely cause similar attenuation bias to my main specification.

[18] California is omitted, as the pre-NPND dropout rate data are not available.

requires further investigation. Column (4) also shows that the Truancy-based NPND policies have a negative effect on AFGR, while the other Behavior-Based states have a positive, but small and insignificant effect; Truancy-based policies, by causing an increase in the dropout rate, also reduce the true graduation rate. However, this does not explain the negative coefficient on the Enrollment-Based policy. The following section discusses the importance of the AFGR, and investigates the potential causes of this AFGR reduction.

4.1 No Pass, No Drive Laws and the AFGR

Upon passage of the No Child Left Behind Act in 2001, states were pushed to improve test scores, attendance rates, and graduation rates nationwide. Each secondary school was required to report various annual measures of Adequate Yearly Progress (AYP) – four test score measures and an on-time graduation rate measure– that would determine the district's and schools' levels and status of federal education funding. From 2005 to 2012, this on-time graduation rate was the AFGR. Given the effects of the Truancy- and Enrollment-based policies on the AFGR, it is crucial to investigate the underlying causes and mechanisms of this AFGR reduction.

Based on the way in which AFGR is calculated, there are two mechanisms by which these No Pass, No Drive policies could be reducing the AFGR in Table 4. The policy could be decreasing the numerator – that is, reducing the number of diplomas handed out each year. This is likely the cause of the large, negative Truancy-Based coefficient, but is unlikely for Enrollment-Based policies; the incentives created by these Enrollment-Based NPND policies would in no way lead me to expect students to respond by failing to graduate. Alternatively, this policy could be increasing the denominator – that is, increasing enrollment in 8th, 9th, and/or 10th grades. In addressing these two possibilities, an important feature of the US education system needs to be mentioned – the so-called "ninth grade bottleneck".

A number of papers have noted that ninth grade retention rates are extremely high in the United States, and have grown drastically over the past 50-60 years. McCallumore and Sparapani (2010) estimate that approximately 22% of students repeat at least some 9th grade classes. This has increased 9th grade enrollment to the point where it is an entirely inaccurate measure of a single grade cohort's size. The AFGR was created, in part, to correct for this very issue; it reduces the impact of 9th grade enrollment on graduation rates by factoring in estimated cohort size, using 8th and 10th grade enrollments to smooth the estimated enrollment base. To identify the effect the No Pass, No Drive policies have on the AFGR, I first consider how these policies affect year-to-year enrollment changes for a particular cohort. Using the CCD enrollment data I construct a number of dependent variables showing year-to-year enrollment changes for a single cohort. For example, the 8th to 9th grade enrollment change is calculated as:

$$\%\ Change\ 8th - 9th_{s,t} = \frac{9th\ Grade\ Enrollment_{s,t} - 8th\ Grade\ Enrollment_{s,t-1}}{8th\ Grade\ Enrollment_{s,t-1}} * 100\%$$

Alternatively, I could use log(Enrollment) as the dependent variable in these regressions. I use the percentage changes in grade enrollment to better account for between-cohort variation in size, and also to more clearly show the lack of promotion out of the 9th grade. Table 2 demonstrates the "9th grade bottleneck" – 9th grade is on average nearly 9% larger than 8th grade, indicating a great number of students repeat the 9th grade at least once. In addition, many students will repeat the 9th grade until they drop out of school. 9th grade retention rates have fallen since the year 2000, likely due to improvements in school quality and the introduction of policies directed at reducing this effect. However, it still remains true that 9th grade enrollments are much larger than would be expected given 8th grade enrollments – 7.9% larger in 2012 – indicating that 9th grade retention is still a problem. Raw dropout counts show that 9th grade dropouts and 11th grade dropouts are roughly equal in number; however, due to compulsory schooling until the age of 16, 9th graders should be unable to drop out of school unless they have repeated a grade.[19] The enrollment changes in Table 2 show that after 9th grade, enrollment numbers fall every year as one would expect. Although the AFGR was created to reduce the bias caused by the 9th grade pooling

[19] *Numbers and Rates of Public High School Dropouts: School Year 2004-05* shows 125,115 9th graders and 125,882 11th graders dropped out of school in 2004-05.

effect, if these policies are causing a disparate impact in any one grade then the AFGR would be still be influenced, even if the "true" four-year graduation rate is unaffected. I begin by directly examining the effect of Truancy- and Enrollment-based NPND policies on the 9th grade bottleneck, and then show that a reduction in graduates causes only the Truancy-based policy AFGR reduction, not the Enrollment-based effect. Table 5 looks at the 9th grade bottleneck, showing results from a regression specified identically to the previous regressions examining dropout and graduation rates, including the same set of controls, but using percentage enrollment changes as the dependent variable.

Table 5 shows important effects of the Enrollment-Based policies. First, note in column (1) that the coefficient on per-student spending is large and negative. This indicates that the 9th grade pooling effect is correlated with low-quality, or at least poorly funded, schooling. Compare this to the Enrollment-Based policy effect; the policy increases the gap between 8th and 9th grade enrollment, and it may make the 9th to 10th grade attrition rate larger as well, although this coefficient is not significant. Clearly, this is evidence that the Enrollment-Based policy increases the rate of pooling in 9th grade, constricting the 9th grade bottleneck to an even greater degree than before the policy was implemented. To understand why this occurs, consider the type of student affected by this policy – before the policy, many students were dropping out of school at the age of 16, but with the policy, they wait until they turn 18. The results in Table 3 indicated that dropout rates were not significantly affected by this policy, so it must be that the policy is simply causing students to delay their dropout decision. Without the policy, many of these "marginal" students – those who would have changed their behavior under these No Pass, No Drive policies – are retained in 9th grade until they drop out at age 16. With the policy, the primary difference is that these marginal students are retained in 9th grade an additional 1 or 2 years, eventually dropping out at age 18.

Additionally, Table 5 verifies the results from Table 3 regarding the effects of Truancy-based policies on dropout rates. The majority of states in the time period examined, including all Truancy-based states, had compulsory schooling until the age of 16, and also allowed students to receive their driver's license at the age of 16. Since students who are progressing through school normally will be 16 years old in the 10th grade, the result in column (3) is entirely expected – truant teens drop out of school at the first available opportunity, making the gap between 10th and 11th grade enrollment larger.

An alternative explanation for the AFGR falling due to the Enrollment-based policies is a decrease in the numerator of the AFGR statistic – that these policies reduce the number of diplomas given out in some meaningful way. I test for this in two ways; first, by using the 12th grade graduation rate, calculated as before as the ratio of diplomas to 12th grade enrollment in a given academic year. I also consider a simple five-year graduation rate – calculated as the ratio of diplomas to 8th grade enrollment five years prior. The five-year graduation rate should not be affected by 9th grade pooling, so while it may be an overall noisier measure of on-time graduation than the AFGR, Enrollment-based policies should not affect this estimate, while Truancy-based policies should. If these policies are truly making students graduate at a lower rate, instead of artificially lowering the AFGR statistic via increasing 9th grade enrollments, the coefficient estimates for these two specifications, especially the five-year graduation rate, should be negative, significant, and equal in magnitude to the estimates in Table 4. Table 6 shows the effects of NPND policies on these two graduation rate estimates.

Column (1) shows the effect on the percentage of 12th graders that graduate in a given year. No policy has a significant effect, but magnitude of the coefficient on Enrollment-based policies is less than 1, while the coefficient on Truancy-based policies is nearly 2.5. However, this is a far less accurate measure of a possible effect of these policies on graduation than the five-year graduation rate. If some of the "marginal" students under Enrollment-based policies – those who would drop out at age 16 without the policy but instead wait until they turn 18 – move through school on-track, meaning they are in 12th grade when they turn 18, then it is reasonable to assume that not only might these students drop out of the 12th grade, but also the average ability of 12th graders would decrease as a result of the policy. A more accurate measure of how these policies affect on-time graduation rates is the five-year graduation rate. Column (3) shows that the Enrollment-Based policy effect is negative, relatively small, and insignificant. Truancy-based policies appear to cause extremely large, but insignificant decreases in the five-year graduation rate, but the other policies do not have an effect. All of this evidence suggests that, while the Truancy-Based policies reduce *actual* graduation rates and increase dropout rates, the Enrollment-Based policies only *artificially* reduce the AFGR by increasing 9th grade enrollment.

Additionally, these results do not directly conflict with the findings on educational attainment in Barua and Vidal-Fernandez's previous work. I find small and insignificant effects on the alternative graduation rate estimates in Table 6, while Barua and Vidal-Fernandez find a small, but significant increase in the proportion of the population with a high school diploma. In Appendix Table A7 I replicate Barua and Vidal-Fernandez's estimation of the effects of NPND policies on high school completion in the ACS 2009-2011, categorizing policies into three groups as in my study, and compare to my analogous results. In the full sample, I find comparable results to Barua and Vidal-Fernandez for Enrollment-based policies.[20] Truancy-based policies appear to have larger effects than Enrollment-based policies, but further investigation in column (2) shows this is driven by California. California's policy is likely to cause similar responses to Enrollment-based policies, as the minimum dropout age in California is 18, meaning students cannot legally exploit the "dropout loophole" available in Delaware, Nevada, and New Mexico.[21] California is missing dropout data before their NPND policy, and as a result is not used for identification in my dropout rate estimation. However, California is not missing in my AFGR and enrollment data; my results presented in Panels B and C of Table A7 are not sensitive to the omission of any individual state with a Truancy-based policy. This suggests that the differences between my findings and those of Barua and Vidal-Fernandez are driven primarily by our classification of policies and choices of outcome variables, and that my study complements theirs, giving a more complete description of the effects

[20] Here I replicate their most restrictive specification, allowing for state-specific linear and quadratic trends.

[21] The minimum dropout age in Delaware is 16, in Nevada is 17, with exemptions given to working teens as young as 14, and in New Mexico is 18, with exemptions given to working teens and teens enrolled in GED study programs.

of NPND policies on teen educational choices.

To demonstrate the effects NPND policies have on grade-by-grade enrollment more clearly, Figure 1 shows, for every 1,000 8th graders, how many students are expected to be enrolled at each grade level.

In Figure 1, the "Without NPND" bars show the average year-to-year enrollment changes among all 23 states that never begin a NPND policy. The "With NPND" bars add the Enrollment- or Truancy-based regression results from Table 5 to the "Without NPND" averages, meaning that the "With NPND" effects show the expected results of a state enacting NPND laws among the states that do not currently have NPND laws. The number atop each bar shows the estimated size of the gap between the states with and without NPND policies. There are a few key effects of the NPND policies that clearly reveal themselves in this graph. First, looking at Graph (a), Enrollment-based NPND laws have the largest effect in 9th grade. For every 1000 8th graders, 9th grade enrollment increases by about 28 students as a result of these policies. Second, the negative coefficient on the 9th to 10th grade enrollment change does not cancel out the large, positive coefficient on the 8th to 9th grade enrollment change – 10th grade enrollment is also larger than expected, given 8th grade enrollment. Finally, the remaining coefficients essentially cancel out the remaining effect. That is, 11th and 12th grade enrollments and diploma counts are essentially identical when comparing NPND states to their counterparts without NPND laws. Additionally, recall that none of the coefficients after the 8th to 9th grade change were significantly different from zero. As a result, Figure 1 (a) may be underestimating the size of 10th, 11th, and 12th grades, as well as the number of diplomas. Overall, the massive increase in 9th grade enrollment and the smaller increase in 10th grade enrollment are much more likely to be the causes of the AFGR reduction than a decline in high school graduates.

Looking at Figure 1 (b), the Truancy-based policies show the opposite effect from the Enrollment-based policies. 9th and 10th grades are unaffected by Truancy-based policies, and there is a substantial decrease in 11th and 12th grade enrollment (20-30 students per 1,000 8th graders) and in the number of graduates caused by these policies. This indicates Truancy-based policies truly cause an increase in the dropout rate, and that many of the teens who drop out because of these policies would have otherwise graduated from high school. There appears to be an additional reduction in the number of graduates, relative to the number of 12th graders. This is likely caused by students who become older than their state's minimum dropout age – in California and New Mexico, the minimum dropout age is 18 (with numerous exemptions in New Mexico), and in Nevada, the minimum dropout age is 17. Given this, it is reasonable to expect students to drop out at their first available opportunity, which occurs in the 10th grade in Delaware or if exempt from compulsory schooling in Nevada and New Mexico, and occurs in the 12th grade otherwise. So while both Enrollment-based and Truancy-based policies decrease the AFGR, Enrollment-based policies are distorting enrollment without truly decreasing the number of graduates, while Truancy-based policies are increasing the number and rate of dropouts and decreasing the true likelihood a teen will graduate.

4.2 Event Studies

A potential concern with these results is the validity of the identifying assumption – that, after removing the effects of the spending and unemployemnt controls and state-specific linear time trends, the dependent variables have parallel trends between states with and states without No Pass, No Drive policies. To test this, I estimate dynamic diff-in-diff models analogous to the following:

$$
\begin{aligned}
Y_{st} =& \beta_0 + \sum_{i=-3}^{-2} \eta_i EnrollStarted_{s,t+i} + \sum_{i=0}^{5} \eta_i EnrollStarted_{s,t+i} + \phi^- EnrollStarted^{4-}_{s,t} + \phi^+ EnrollStarted^{6+}_{s,t} \\
& \beta_2 Truancy_{s,t} + \beta_3 Behavior_{s,t} + \beta_4 log(Spending)_{s,t} + \beta_5 Unemp_{s,t} + \delta_t + \gamma_s + \gamma_s * t + \varepsilon_{s,t}
\end{aligned}
$$

In the equation above, the terms $EnrollStarted_{s,t+i}$ are binary, equal to 1 i years *after* the Enrollment-based policy was first implemented in state s. The terms $EnrollStarted^{4-}_{s,t}$ and $EnrollStarted^{6+}_{s,t}$

are binary, equal to 1 when the policy begins in 4 or more years[22] or has been in place 6 or more years, respectively. This model normalizes the coefficient 1 year before the policy begins to zero, for ease of visual interpretation. I collect the coefficient estimates η_t and their standard errors and graph them in a series of plots in Figures 2 and 3. I also estimate the above equation without including state-specific linear time trends ($\gamma_s * t$), and present the estimates in Figure 4

In Figure 2, the parallel paths assumption holds well for the dropout rate, as the pre-policy trend is flat and near zero. Additionally, following implementation of the Truancy-based NPND policies, the dropout rate rises substantially. For the AFGR and 10th-11th grade enrollment changes, the pre-policy trend is somewhat concerning. However, if I omit state-specific linear trends as in Figure 4, the pre-policy trend for the AFGR is flat and near zero, and the pre-policy trend for the dropout rate is concerning. Considering both of these results together, both model specifications suggest the same effects of Truancy-based NPND policies on event dropout rates and the AFGR, with the strength of my identification varying depending on the specification used.

In Figure 3, the parallel paths assumption holds well for both the dropout rate and the AFGR – Enrollment-based NPND policies appear to not affect the annual dropout rate, but decrease the AFGR. The 8th-9th grade enrollment change is potentially concerning, as the preexisting trend appears to continue after the policy begins, but the combination of a decline in the AFGR and no change in dropout rates suggest early high school retention as the most likely cause. In Figure 4(d), the pre-policy noise in estimation for the AFGR makes identification difficult, as the states with Enrollment-based NPND policies do not appear to satisfy the parallel trends assumption. Overall, comparing Figures 2 and 3 to Figure 4 shows why I prefer a specification with state-specific linear trends to one without trends. I find qualitatively and quantitatively similar effects of Truancy-based policies regardless of which specification I choose, and state-specific linear trends allow proper identification of the Enrollment-based policies' effects on the AFGR.

[22] This is chosen primarily to avoid endpoint effects. For Truancy-based policies, I do not have AFGR or enrollment data for California for 5+ years before the policy begins. For enrollment policies, 3 of the 20 states do not have data 5 or more years before the policy begins.

5 Conclusion

Overall, No Pass, No Drive policies do not appear to be as unambiguously beneficial to educational outcomes as previous studies have found. Of immediate concern to policymakers should be the effects of Truancy-Based policies, which I am the first to separately identify and study, on teen decision-making. These policies distort incentives, making absence from school much more costly without directly altering the costs of dropping out of school. As a result, Truancy-based policies result in a massive increase in the annual dropout rate, and an associated decrease in the four-year graduation rate, as well as 11th and 12th grade enrollment. Important to note is that the students who drop out of school as a result of these policies are ones who likely would have completed high school without this policy intervention – the dropout rate rises and the true graduation rate falls, indicating that these new dropouts would have completed high school if not for the policy intervention. This means that Truancy-based NPND policies have strong implications beyond the contemporaneous education effects, and in fact beyond the teenage years into adulthood. Also important to note is the magnitude of the impact of these policies; I calculate that Truancy-based policies increase the number of dropouts in the United States by over 27,000 teens *per year*,[23] showing that, despite the relative rarity of these policies, the overall effect is quite large.

Looking at Enrollment-based No Pass, No Drive policies, the effects are more subtle, yet still quite important. While my analysis is unable to refute previous results that educational attainment increases due to the enactment of Enrollment-based NPND policies, this increase seems to, at most, marginally improve the number of high school graduates in the population, and does not seem to improve the number of *on-time* high school graduates. This slight increase in educational attainment also carries with it some significant, but previously unnoticed, costs. Enrollment in 9th grade, relative to 8th grade, increases by nearly 3 percent, and enrollment in 10th grade increases by approximately 1 percent. This puts pressure on state and local education budgets, as higher student enrollment numbers would require the hiring of additional teachers and staff, as well as

[23]High school enrollment for California, Delaware, Nevada, and New Mexico in 2012-13 was about 2.2 million, multiplied by the Truancy-based coefficient from Table 3.

increased overall spending on student services and needs ranging from cafeteria lunches to textbook purchases. Additionally, the students being retained in school as a result of these NPND policies are the "marginal dropouts" who are nearly indifferent between dropping out of school and remaining in school in the absence of the NPND policies. These students likely have an increased prevalence of behavioral and learning issues, which may be imposing negative externalities on their peers (Carrell and Hoekstra 2010), raising the cost of NPND policies by reducing educational quality for the students who otherwise have their behavior unaffected by these policies. Add to this the fact that, for nearly a decade, NPND policies increased the likelihood for schools to fail to meet their Adequate Yearly Progress standards under the No Child Left Behind Act, and Enrollment-based NPND policies do not appear to be as unambiguously beneficial as previously thought.

Moving forward, policymakers need to reevaluate the goals of No Pass, No Drive policies and examine how the results of these policies align with their goals. If the goal is simply to keep teenagers in school, potentially causing improvements in outcomes such as juvenile crime rates (Barua and Vidal-Fernandez 2016), then Enrollment-based NPND laws appear to successfully achieve this goal – 9th and 10th grade enrollment increases, without bringing associated decreases in 11th or 12th grade enrollment. However, if the goal of NPND policies is to improve students' educational outcomes, then a revision of these policies is needed. Although Enrollment-based NPND policies increase educational attainment for marginal dropouts, they likely impose substantial costs on their schools and the overall student population; Truancy-based NPND policies cause teens to drop out of school, but potentially reduce costs on schools and the overall student population. Enrollment-based NPND policies increase retention by incentivizing enrollment and attendance, Truancy-based policies directly decrease enrollment by inducing students to drop out of school, and yet neither fully achieves its stated goal. In an interview with the New York Times, the West Virginia official in charge of enforcing the nation's first NPND policy stated, "We expect our graduation rate to climb steadily from here on out... Ultimately, that means we're going to have a better educated labor force" (Ayres 1989). NPND policies, in their current form, are weakly effective at achieving these goals.

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Table 1: No Pass, No Drive Policies in the United States

State Name	Policy Enactment Year	Hardship/Working Exemption	GED Exemption	Other
Enrollment-Based				
Alabama	1993	Yes	No	Teen parents exempt
Arkansas	1989	Yes	No	
Florida	1997	Yes	Yes	
Georgia	1998	No	No	
Idaho	1996	No	No	
Illinois	2007	No	No	
Indiana	1991	Yes	No	
Iowa	1994	No[24]	No	
Kentucky	1990, 2007[25]	Yes	No	
Louisiana	2009	No	No	
Mississippi	1994	No	No	
North Carolina	1998	Yes	No	
Ohio	1992	No	Yes	
Oklahoma	1996	Yes	No	
Oregon	2000	No	Yes	
South Carolina	1998	No	No	
Tennessee	1990	No	No	
Texas	1995	No	No	
West Virginia	1988	No	No	
Wisconsin	1988	No	No	Only some counties enacted policy
State Name	Policy Enactment Year	Truants Only	Discipline Only	Other
Truancy-Based				
California	1992	Yes	No	
Delaware	2000	Yes	No	
Nevada	2003	Yes	No	
New Mexico	2005	Yes	No	
Behavior-Based				
Kansas	1999	No	Yes	
Louisiana	2004[26]	No	Yes	
Oregon	1995[26]	No	Yes	
Rhode Island	2005	No	Yes	
Virginia	1996	No	No	Exempt with parental permission

[24]In the original version of this policy, Iowa had an exemption for working students. This exemption was removed in 2005.

[25]Kentucky instituted this policy in 1990 in some counties; however, the law was struck down by the Kentucky Supreme Court in 2003 (*D.F. v Codell*). A new law, identical to the original for the purposes of this research, was implemented in 2007.

[26]Louisiana and Oregon initially had Behavior-Based policies focusing on discipline, but then changed to Enrollment-Based policies focusing on dropouts at a later date.

Table 2: Summary Statistics

Variable	Mean	St. Deviation	Min	Max	N
Dependent Variables					
Dropout Rate	4.42%	1.90%	1.1%	13.7%	869
AFGR	75.69%	8.15%	53.47%	97.72%	1018
% Change 8th-9th	8.79%	6.47%	-10.57%	47.10%	1326
% Change 9th-10th	-7.24%	6.02%	-38.16%	13.84%	1326
% Change 10th-11th	-7.96%	5.54%	-35.90%	9.00%	1326
% Change 11th-12th	-6.16%	5.37%	-38.63%	9.97%	1326
12th Grade Grad. Rate	90.44%	6.03%	63.00%	100% (cut off)	1171
5-Year Grad. Rate	77.45%	8.31%	53.92%	100% (cut off)	1018
Control Variables					
Unemp. Rate	5.61%	1.88%	2.3%	13.7%	1122
Per-Student Spending	$7620.13	$2853.20	$2767	$20910	1071

Table 3: Effects of No Pass, No Drive Policies on Dropouts

	Dropout Rate				
	(1)	(2)	(3)	(4)	(5)
Enrollment-Based	-0.929**	-0.446	0.125	0.272	0.174
	(0.351)	(0.449)	(0.346)	(0.375)	(0.364)
Other Policies	-0.250	0.538	1.018***	1.050***	
	(0.513)	(0.536)	(0.295)	(0.276)	
Truancy-Based					1.229**
					(0.263)
Behavior-Based					0.393
					(0.648)
Fixed Effects		Yes	Yes	Yes	Yes
State-Specific Trends			Yes	Yes	Yes
School and Macro Controls				Yes	Yes
N	869	869	869	818	818

Standard errors in parentheses, clustered by state.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Behavior- and Truancy-based p-values are bootstrapped as in Cameron et al. (2008)

"Fixed Effects" refers to both state and year fixed effects. "School and Macro Controls" are controls for log(per-student spending) and unemployment rate.

Table 4: Effects of No Pass, No Drive Policies on AFGR

	Averaged Freshman Graduation Rate				
	(1)	(2)	(3)	(4)	(5)
Enrollment-Based	-3.667*	-0.127	-1.133	-1.330*	-1.139
	(1.932)	(0.685)	(0.816)	(0.763)	(0.764)
Other Policies	-4.949**	-2.412	-2.628*	-2.732*	
	(2.244)	(1.782)	(1.461)	(1.448)	
Truancy-Based					-4.999*
					(2.365)
Behavior-Based					0.597
					(1.103)
Fixed Effects		Yes	Yes	Yes	Yes
State-Specific Trends			Yes	Yes	Yes
School and Macro Controls				Yes	Yes
N	1018	1018	1018	1018	1018

Standard errors in parentheses, clustered by state.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Behavior- and Truancy-based p-values are bootstrapped as in Cameron et al. (2008)

"Fixed Effects" refers to both state and year fixed effects. "School and Macro Controls" are controls for log(per-student spending) and unemployment rate.

Table 5: Effects of No Pass, No Drive Policies on Grade Enrollments

	Percentage Change in Enrollment			
	8th-9th	9th-10th	10th-11th	11th-12th
	(1)	(2)	(3)	(4)
Enrollment-Based	2.792**	-1.651	-0.653	0.333
	(1.281)	(1.040)	(0.710)	(0.539)
Truancy-Based	-0.0158	-0.169	-2.769*	0.859
	(2.021)	(1.927)	(1.677)	(1.434)
Behavior-Based	-0.260	1.673	-1.444	0.955
	(0.748)	(0.671)	(1.119)	(0.565)
log(Spending)	-14.43**	3.864	-1.659	-5.341*
	(6.241)	(6.248)	(5.989)	(3.041)
Unemp. Rate	-0.575	-0.177	-0.166	-0.0017
	(0.352)	(0.244)	(0.305)	(0.196)
N	1071	1071	1071	1071

Standard errors in parentheses, clustered by state

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Behavior- and Truancy-based p-values are bootstrapped as in Cameron et al. (2008)

All specifications include state and year fixed effects, controls for log(per-student spending) and unemployment rate, and state-specific time trends.

Table 6: Effects of No Pass, No Drive Policies on Alternative Graduation Rates

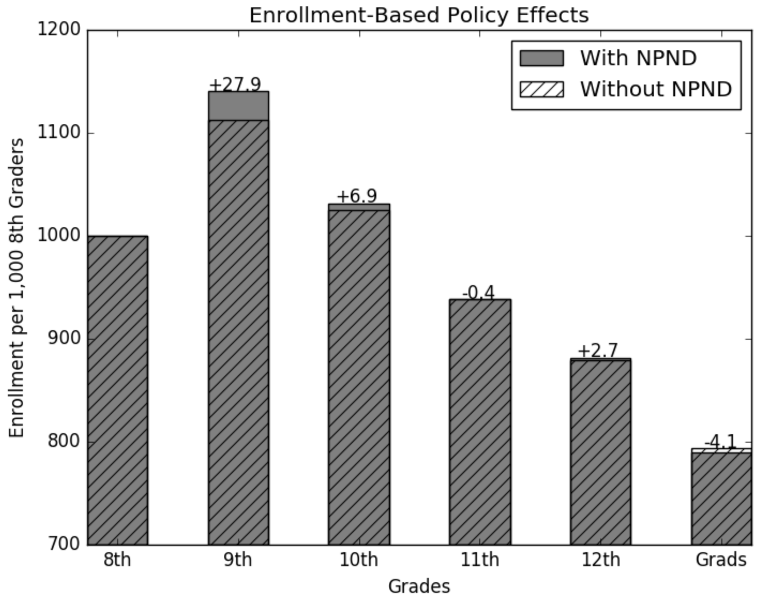
	12th Grade Grad. Rate		5-Year Grad. Rate	
	(1)	(2)	(3)	(4)
Enrollment-Based	-0.829	-0.747	-0.738	-0.479
	(1.093)	(1.086)	(0.729)	(0.724)
Other Policies	-1.230		-3.657	
	(0.719)		(1.827)	
Truancy-Based		-2.488		-6.422
		(1.065)		(2.981)
Behavior-Based		0.078		0.0187
		(1.167)		(0.862)
N	1018	1018	1018	1018

Standard errors in parentheses, clustered by state

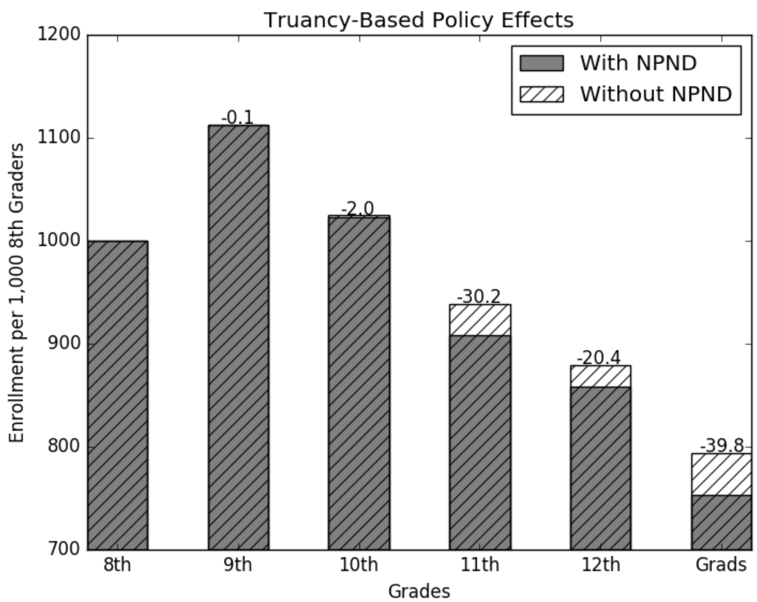
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Behavior- and Truancy-based p-values are bootstrapped as in Cameron et al. (2008)

All specifications include state and year fixed effects, school and macro controls, and state-specific time trends.



(a) Enrollment-Based Policies

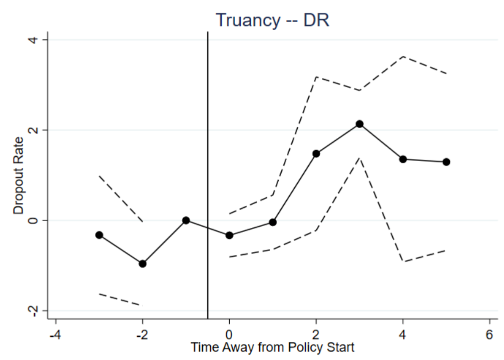


(b) Truancy-Based Policies

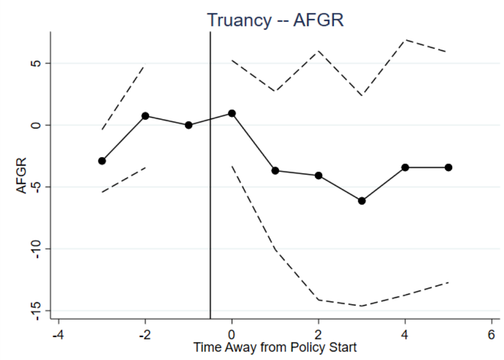
NOTE: The hashed blocks above show average enrollment and diplomas per 1,000 8th graders for states without NPND policies. The solid blocks add the estimates from Table 5 to these averages.

Figure 1: No Pass, No Drive Enrollment Effects

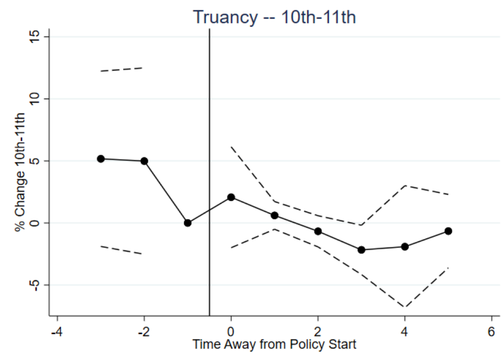
Figure 2: Truancy-Based Policy Trends



(a) Dropout Rate



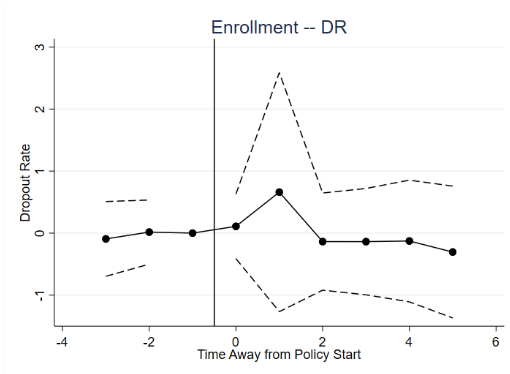
(b) AFGR



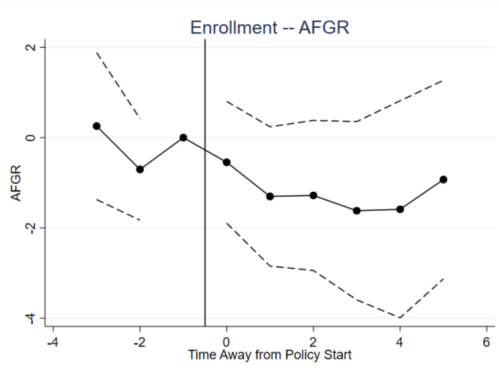
(c) % Change 10th-11th

NOTE: Time=0 is the first year of the policy. A vertical line appears at Time=-0.5, so that all points lying to the left are untreated, and all points lying to the right are treated. Year-by-year point estimates and 95% confidence intervals are displayed.

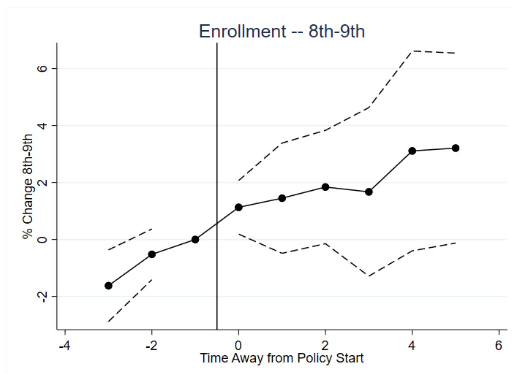
Figure 3: Enrollment-Based Policy Trends



(a) Dropout Rate



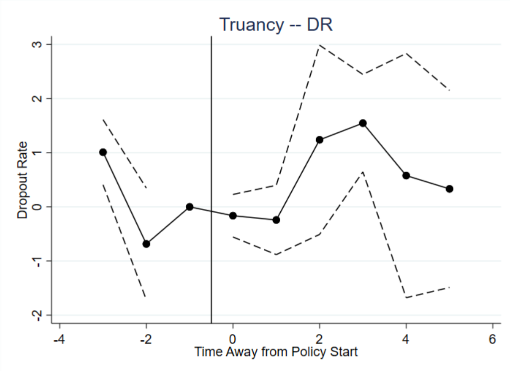
(b) AFGR



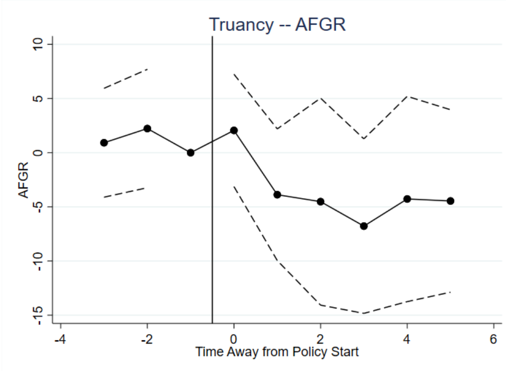
(c) % Change 8th-9th

NOTE: Time=0 is the first year of the policy. A vertical line appears at Time=-0.5, so that all points lying to the left are untreated, and all points lying to the right are treated. Year-by-year point estimates and 95% confidence intervals are displayed.

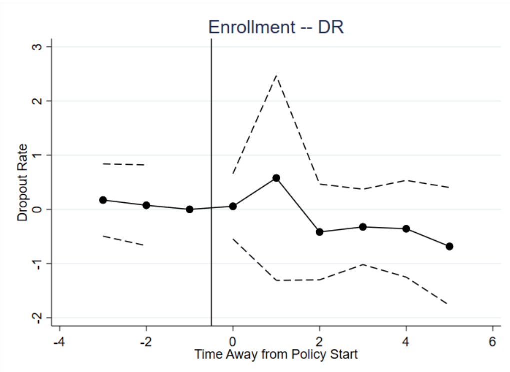
Figure 4: Policy Effects, Without Linear Trends



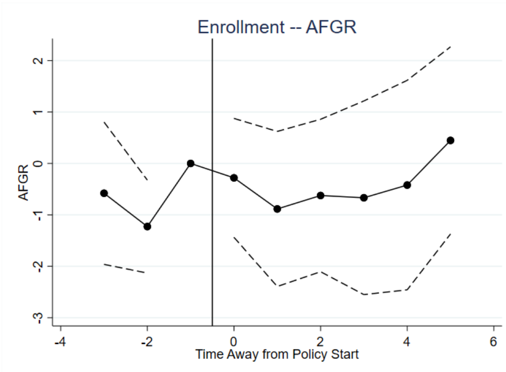
(a) Truancy – Dropout Rate



(b) Truancy – AFGR



(c) Enrollment – Dropout Rate



(d) Enrollment – AFGR

NOTE: Time=0 is the first year of the policy. A vertical line appears at Time=-0.5, so that all points lying to the left are untreated, and all points lying to the right are treated. Year-by-year point estimates and 95% confidence intervals are displayed.

Appendix: Placebo Testing and Bootstrapping

A possible concern with my results is that the data may be structured in a way that causes a high rate of type I error. For example, data with an extremely high level of variation in the dependent variables could cause overrejection of the null hypothesis, and thus any regression results would be largely uninterpretable. Bertrand et al. (2004) demonstrate that difference-in-difference estimation often suffers from overrejection of the null hypothesis, caused by serial correlation in the dependent variable. Since my dependent variable certainly has serial correlation, this could be causing my standard errors to be too small, creating type I error. To examine this problem, I mimic the methodology of Bertrand et al., by creating a group of "placebo policies," where the policies are randomly assigned to various states, but in a similar way to how they are actually distributed across the United States. For Enrollment-Based policies, each state is given a 40% chance to begin the placebo policy so that in expectation, the number of states with the policy will be 20, the same as the number of Enrollment-Based states. In testing Truancy-Based policies, states are given an 8% chance to begin the placebo policy. After these states are selected, a "policy enactment year" is randomly selected from the discrete uniform distribution, with the earliest possible start year being 1987. Then, regression models identical to my main specifications are run and the placebo policy treatment coefficient is tested at the same significance level of my actual result to see the degree to which my results are at risk of type 1 error. In addition, I test for symmetry by performing both 1- and 2-tailed significance tests. If the data are structured in a way that causes overrejection of the null hypothesis in only one direction, this could also be cause for concern. For regression models where I did not find significance, I test for type II error by running these models and testing the placebo policy treatment coefficient at the $p<0.1$ level. I simulate these placebo policy assignments for 10,000 replications, and calculate the simulated rejection rate I observe. The results of these placebo tests are summarized in Table A1.

The Enrollment-Based placebo testing yielded no unexpected results – in all regression models, the rejection probability was extremely close to its expected level. However, the Truancy-Based placebo testing demonstrated severe type I error – testing dropout rates at the 1% significance level, the null hypothesis was rejected over 20% of the time. All other measures also showed high rates of rejection for Truancy-Based policies. This indicates that using OLS standard errors is inappropriate for this analysis. The Enrollment-Based policy results, however, do not seem to face this problem, and are therefore unlikely to be driven by pure chance.

To correct for the type I error demonstrated by my placebo testing, I follow the methodology of Cameron et al. (2008) and use the wild restricted cluster bootstrap t-statistic for statistical inference on Truancy-based policies. In their analysis, Cameron et al. test the setup of Bertrand et al. (2004), and show that by using the wild cluster bootstrap-t, they can achieve rejection rates with little to no Type I error. In performing this test, the standard cluster-robust variance estimate is used to calculate the standard errors, but the Wald statistic generated from this estimate is tested against the bootstrap distribution of Wald statistics. Table A2 shows the Wald statistic for each regression, and the p-value from testing this against the bootstrap Wald distribution (B=10,000).

All Truancy-based policies in the main body of the paper reflect these bootstrapped p-values. From these results, three of the seven main regressions in my analysis show statistical significance for at least $p < 0.1$, and two, the 5-Year Graduation Rate and the 12th Grade Graduation Rate, are statistically significant in the typical OLS Wald test but are not significant when performing wild cluster bootstrap-t testing.

Table A1: Placebo Policy Testing Results

	Significance Level (Tails)	Rejection Percentage
Enrollment-Based – 40% Chance to Begin Placebo Policy		
Dropout Rate	0.1 (2)	11.19%
AFGR	0.1 (2)	11.27%
AFGR	0.05 (1)	5.68%
12th Grade Grad. Rate	0.1 (2)	10.90%
5-Year Grad. Rate	0.1 (2)	10.85%
Percentage Changes		
8th-9th	0.05 (2)	4.94%
8th-9th	0.025 (1)	2.63%
9th-10th	0.1 (2)	10.35%
10th-11th	0.1 (2)	10.38%
11th-12th	0.1 (2)	10.78%
Truancy-Based – 8% Chance to Begin Placebo Policy		
Dropout Rate	0.01 (2)	21.05%
Dropout Rate	0.005 (1)	8.93%
AFGR	0.05 (2)	26.21%
AFGR	0.025 (1)	12.05%
12th Grade Grad. Rate	0.05 (2)	24.18%
12th Grade Grad. Rate	0.025 (1)	11.47%
5-Year Grad. Rate	0.1 (2)	25.44%
5-Year Grad. Rate	0.05 (1)	11.61%
Percentage Changes		
8th-9th	0.1 (2)	30.13%
9th-10th	0.1 (2)	31.90%
10th-11th	0.1 (2)	25.45%
11th-12th	0.1 (2)	30.39%

NOTE: All rejection percentages are for 10,000 replications.

Table A2: Bootstrapping Results

Dependent Variable	t-statistic	p-value
Dropout Rate	5.90	0.011
AFGR	-2.36	0.071
% Change 9th-10th	0.60	0.603
% Change 10th-11th	-1.90	0.074
% Change 11th-12th	-0.56	0.627
5-Year Grad. Rate	-2.15	0.307
12th Grade Grad. Rate	2.43	0.522

All specifications test Truancy-Based policies.

NOTE: All Behavior-Based policies were not significant at the $p < 0.1$ level, and are omitted from this table.

Appendix: Additional Tables and Figures

Table A3: Teen Driving Status by Grade

	9th Grade	10th Grade	11th Grade
Drive on Their Own	10%	33%	67%
Learning to Drive	44%	50%	23%
Affected by NPND	**54%**	**83%**	**90%**
Do not Drive	46%	17%	10%

SOURCE: Driving Through the Eyes of Teens, A Closer Look. 2009.

Table A4: No Pass, No Drive Enrforcement, 2009-10 School Year

State	Type of Enforcement	Number	Total Teen Drivers (Under Age 18 in 2009)
Florida – 3rd Quarter ONLY			343,250
	Notices of Ineligibility for License Issued	7,557	
	Notices of Intent to Suspend License Issued	2,219	
	Suspensions Issued	1,844	
	Second Suspensions Issued to Repeat Violators	100	
Georgia	Suspensions Issued	16,000	91,179
Kentucky	Suspensions Issued	2,317	20,220
Tennessee	Suspensions Issued	3,697	132,210

SOURCE: Columns 2-3 Strengthening Attend 'n' Drive Laws to Reduce Truancy and Dropouts (2011).
Column 4 US Federal Highway Administration (2009).

Table A5: Donut Treatment Effects on Event Dropout Rates

	Dropout Rate	
	Excluding 1st Year	Excluding 1st Year and California
Enrollment-Based	0.120	0.123
	(0.398)	(0.398)
Truancy-Based	1.979**	1.990**
	(0.544)	(0.543)
Behavior-Based	0.639	0.640
	(0.227)	(0.227)
N	816	806

Standard errors in parentheses, clustered by state

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Behavior- and Truancy-based p-values are bootstrapped as in Cameron et al. (2008)

Table A6: Effects on AFGR and Enrollment,
Excluding Louisiana and Oregon

	(1) AFGR	(2) 8th-9th Enrollment Change
Enrollment-Based	-1.081	2.465*
	(0.807)	(1.247)
Truancy-Based	-4.464*	-2.206
	(3.354)	(1.362)
Behavior-Based	0.622	-0.296
	(1.143)	(0.804)
N	978	1029

Standard errors in parentheses, clustered by state

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

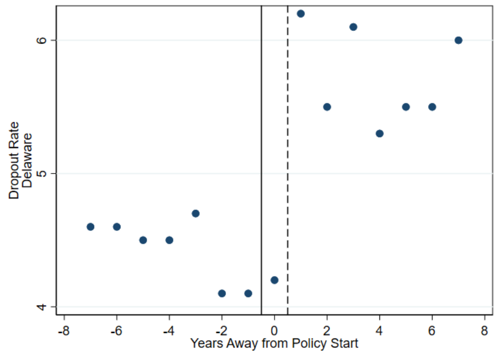
Behavior- and Truancy-based p-values are bootstrapped as in Cameron et al. (2008)

Table A7: Replication of Barua and Vidal-Fernandez

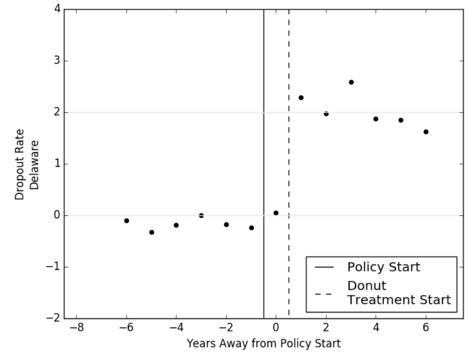
	(1) All States	(2) Omit CA	(3) Omit DE	(4) Omit NV	(5) Omit NM
Panel A: BVF Data – High School Completion					
Enrollment	0.00947*	0.0120**	0.00955*	0.00962*	0.00958*
	(0.00564)	(0.00541)	(0.00565)	(0.00566)	(0.00564)
Truancy	0.0336***	-0.0272	0.0342***	0.0341***	0.0334***
	(0.00484)	(0.0181)	(0.00527)	(0.00514)	(0.00493)
Behavior	-0.00547*	-0.00201	-0.00557*	-0.00534	-0.00546*
	(0.00324)	(0.00260)	(0.00329)	(0.00331)	(0.00324)
N	1821191	1636657	1817644	1817509	1811864
Panel B: NCES Data – Dropout Rate					
Enrollment	0.174	0.178	0.160	0.182	0.150
	(0.364)	(0.364)	(0.378)	(0.365)	(0.371)
Truancy	1.229**	1.236***	0.974***	1.290***	1.239***
	(0.263)	(0.263)	(0.111)	(0.324)	(0.355)
Behavior	0.393	0.389	0.366	0.342	0.355
	(0.648)	(0.652)	(0.628)	(0.656)	(0.664)
N	818	808	799	799	798
Panel C: NCES Data – AFGR					
Enrollment	-1.139	-1.108	-1.151	-1.051	-1.207
	(0.764)	(0.775)	(0.769)	(0.742)	(0.772)
Truancy	-4.999*	-5.969**	-5.545*	-2.843*	-6.194**
	(2.365)	(2.466)	(2.770)	(1.695)	(2.570)
Behavior	0.597	0.615	0.586	0.563	0.561
	(1.103)	(1.158)	(1.109)	(1.141)	(1.122)
N	1018	998	998	998	998

Standard errors in parentheses, clustered by state of birth (Panel A) or state of residence (Panels B, C)

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

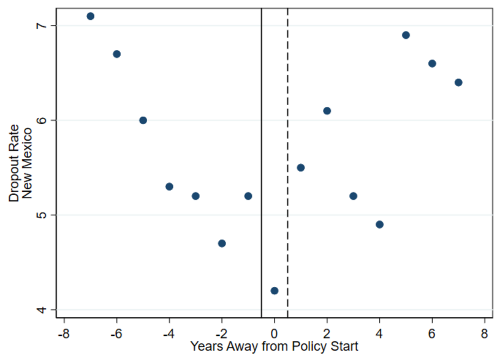


(a) No Controls

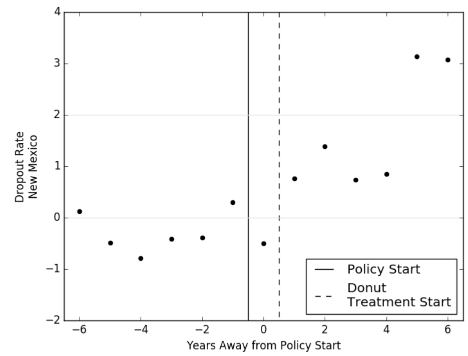


(b) Residual of Controls

Delaware

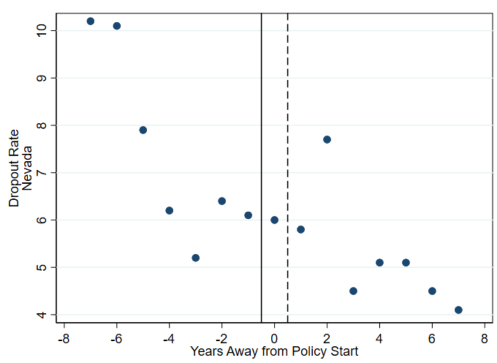


(c) No Controls



(d) Residual of Controls

New Mexico



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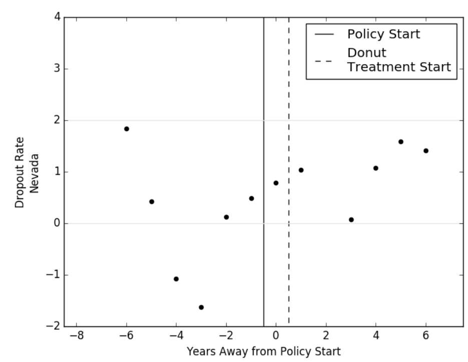

(f) Residual of Controls

Nevada

Figure A1: Pre- and Post-Policy Dropout Rate Trends for Truancy-based States